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PHILOSOPHICAL TRANSACTIONS.

September 12. 1670.

The CONTENTS.

The Continuation of the Experiments concerning Respiration, promised in the precedent Tract, and communicated by the same Noble Author, in Ten other Titles; viz. Of the Accidents, that happen'd to Animals in Air, brought to a Considerable degree of Rarefaction; To which is annex'd a digressive Experiment concerning Respiration upon very high Mountains. Of the Observations produced in an Animal by Changes as to Rarity and Density made in the self-same Air. Of an unsuccessfull Attempt, to prevent the Necessity of Respiration by the Production of Growth of Animals in the Vacuum Boylianum; together with two digressive Experiments concerning the Expansion of Blood, and other Animal juices, as also other soft parts, even in cold Animals. Of the power of Assuefaction to enable Animals to hold out in Air, by rarefaction made unfit for Respiration. Some Experiments shewing, that Air, become unfit for Respiration, may retain its wonted Pressure. Of the use of the Air to elevate the Steams of Bodies. Of the long continuance of a Slow-worm and a Leech alive in the Vacuum. Of what happen'd to some Creeping Insects; and of the Phenomena suggested by Winged Insects, in the same. Of the Necessity of Air to the motion of such small creatures, as Ants, and even Mites themselves. An Account of two Books: I. TRACTS written by the Honourable Rob. Boyle, concerning Cosmical Qualities; Subterranean and Submarine Regions; the Bottom of the Sea; together with an Introduction to the History of particular Qualities. II. CATALOGUS PLANTARUM ANGLIÆ, operâ Johannis Ræij. M. A. & Soc. Regiæ.

A Preface concerning these Experiments.

Though, to shun prolixity, the Preface, which the Author had made to all he wrote about Respiration, have been purposely omitted; yet there are some few points so necessary to be taken notice of, that 'tis thought unfit to leave them wholly untouch'd. For, the following Experiments being not at first written for the Press, and thrown by for many years, till they were very hastily gather'd together, and in some places supply'd with others, little less hastily annex'd to make some necessary supply, the Reader must not expect in such a Casual Tract (which the Author confesses to be one of the most imperfect and immethodicall of all his compositions) anything but Novelty, and Truth, and an Earnest desire to be serviceable in an Inquiry so important to Mankind, to the Curious in general, and especially to Physicians, who by the encouraging mention they have made of his former endeavours in this kind, have invited him to add these many new Experiments to those few, they had hitherto exercised their Wits upon; And, to leave them the more freedom to do so, he purposely forbore to confirm, or confute any Hypothesis, or so much as propose any of his own; declaring it to be his Aime, not to espouse or make a party, but to communicate to the Curious some matters of fact, that are new; and in an historical way impartially delivered. No more of Preface is now to be added, but that 'tis thought fit, for prevention of Ambiguity, to give this Advertisement touching the ground of the Title of Vacuum Boylianum, to be met with in these Experiments; That as Learned men, both English and Forreigners, in their writings have familiarly for distinction sake employ'd the Titles of Machina Boyliana, and Experimenta Boyliana; so the Author, that writ these, for the most part in hast, and for his own memory, did, for dispatch sake, call the absence of the Air procur'd in his Receivers, our Vacuum; whence by Analogy was fram'd the Vacuum Boylianum, which he therefore thinks the less improper, because to call it Vacuum absolutely, would be judged by many a declaring himself a Vacuist, who does not yet own the being either of their opinion, or a downright Plenist; or else he must be troublesome to the Reader and himself, by frequently explaining, what sort of Vacuum he understands; whereas he declares once for all, that by the Vacuum Boylianum he means such a Vacuity or Absence of Common Air, as is wont to be effected or produc'd in the operations of the Machina Boyliana.

The XI. Title.

Of the Accidents that happen'd to Animals in Air brought to a considerable degree, but not near the utmost one, of Rarefaction.

IN the Generality of our Pneumatical Experiments upon Animals, it suited with our purposes, to rarify the Air as much, and for the most part as fast, as we could ; but I had other Tryals in Design, wherein an extraordinary degree of Rarefaction, but yet not near the highest to which the Air might be brought by our Engin, seem'd likeliest to conduce to my inquiries, and particularly seem'd hopefull to afford some light in reference to those Diseases and Distempers, that are thought primarily to affect the Respiratory Organs, or to depend upon something amiss in Respiration.

Wherefore having *Gages*, by the help of which such Experiments might be much better perform'd then else they could, I attempted several of them ; some of whose successes I find in the following Memorials.

Experiment 1.

A Linote being put into a Receiver, capable to hold about $4\frac{1}{2}$ pints of water, the Glas was well clos'd with Cement and a Cover;
August 16th. but none of the Air was drawn out with the Engine, or otherwise. And though no new Air was let in, nor any change made in the imprison'd Air; yet the Bird continu'd there 3 hours without any apparent approach to death: And though it seem'd somewhat sick, yet being afterwards taken out, it recover'd and liv'd several hours.

Experiment 2.

From the abovemention'd Receiver about half the Air was drawn out, a *Linote* being then in the Glas, and in that rarify'd
August 18th. Air (which appear'd by a *Gage* to continue in that state) the Bird liv'd an hour and near a quarter before it seem'd in danger of death; after which the Air being let in without taking off the Receiver, she manifestly recover'd, and leap'd against the side of the Glas; being taken out into the open Air she flew out of my hand to a pretty distance.

Experiment 3.

We convey'd into a Receiver, capable to hold about $4\frac{1}{2}$ pints of water, a *Larke*, together with the *Gage*, by the help whereof we pump'd out of the Receiver $\frac{1}{4}$ of the Air that was in it before; then
Septemb. 9th. heedfully observing the Bird, we perceived it to pant very much, so that a Learned Physician (from whom I yet dissented,) judg'd those beatings to be Convulsive: Having continu'd thus for a little above a minute and a half; the Bird fell into a true convulsive Motion, that cast it upon the back. And although we made great hast to let in the Air; yet before the Expiration of the Second minute, and consequently in less than half a minute from the time immediately preceding the Convulsion, the Lark was gone past all recovery, though divers means were us'd to effect it.

Ex-

Experiment 4.

Presently after we put into the same Receiver a *Greenfinch*, and having withdrawn the Air till it appear'd by the Gage there remain'd but half, we presently began to observe the Bird, and took notice, that, within a minute after, she appear'd to be very sick, and shaking her head, threw against the inside of the Glass a certain substance which I took to be Vomit, and which afterwards appear'd so; upon this Evacuation the Bird seem'd to recover, and continue pretty well (but not without panting) till about the end of the fourth Minute, at which growing very sick, she Vomited again (shaking her head as at first,) but much more unquestionably then before, and soon after, eat up again a little of her Vomit; at which time (whether that contributed to her Recovery or no) she very much recovered. And tho she had, in all, three fits of Vomiting; yet for the last seven or eight minutes that we kept her in the Receiver, she seem'd to be much more lively then was expected: which may in part be attributed to a little Air that by an accident got in, tho it were immediately pump'd out again. At the end of a full quarter of an hour from the first Exhaustion of the Receiver, the Bird appearing not likely to dye in a great while, and the Engine being needed for other Uses, we took out the Bird and thereby put a period to the Experiment.

Septemb. 9th.

Experiment 5.

I now thought it fit to try. Whether, though a Viper would not hold out very many hours in Air brought to as high a Rarefaction, as we could bring it by our Engin, yet to that cold and vivacious Animal, a very small proportion of Air, in comparison of what was necessary to hot Animals, would not suffice to keep it alive for a considerable time: The Narration of the Experiment I find registred as follows.

A Viper lately bought of the person, that at this season uses to take new ones, almost from day to day, was included together with a Gage in a portable Receiver, capable to hold about $3\frac{1}{2}$ pints of water. This vessel being exhausted, and secured against the regrefs of the Air, the imprisoned Animal was observed from time to time; and observed not only to be alive, but nimbly to put out and to draw back its tongue about 36 hours after it was first shut up; for which reason we continued the Vessel longer in the same shady place; where at the end of 60 hours looking upon her, as I was going to bed, she appeared very dull and faint, and not likely to live much longer: And the next morning being by some occasions carried abroad, and coming to look upon the Glass presently after dinner, I found her stark dead with her mouth open'd to a strange wideness; wherefore suffering water to be impelled by the outward Air into the cavity of the Receiver, to observe how far that vessel was then emptied of Air, we found by the water that was driven in, and afterwards poured out again and measured, that 4 parts of 5, or rather 5 of 6 of the vessel'd Air (if I may so call that which was shut up in the Receiver) had been pump'd out: So that in an

April. 12th.

Air so rarify'd as to expand it self to 5 or 6 times its former and usual dimensions, our Viper was able to live 60 hours, that we are sure of, and perhaps might a pretty while longer.

A digressive Experiment Concerning Respiration upon very high Mountains.

TO illustrate what I have taken notice of in the Printed Experiments about the unsuitness for Respiration, observ'd by the learned *Acosta* in the high mountains of *Pariacaca*, I shall here add, what I have had the curiosity and occasion to learn from divers Travellers, whom I purposely consulted about these matters; whereof you will easily believe that not many of them have had opportunity to give Accounts. Meeting with an Ecclesiastical Person that had visited those high mountains of *Armenia*, (on one of which, because of their height, the Tradition of the Natives will needs have the *Ark* to have rested;) I ask'd him, whether those Mountains are really so high as is given out, and whether at the top of that he visited he found any difficulty of *Breathing*. To the first part of which Question he answer'd; That they were really exceeding high (which he might well judge of, having been upon some of the most famous both in *Europe*, *Asia*, and *Africa*;) and that he could not come to the top, because of the unpassable snows: And to the second part he reply'd, that whilest he was in the upper part of the Mountain he plainly perceiv'd, that he was reduc'd to fetch his breath much oftner than he was wont, and than he did before he ascended the Hill, and after he came down from it. And upon my inquiring, whether or no that difficulty of breathing might not be accidental, or peculiar to him, he told me that he himself having express'd some wonder to find himself so short-winded, the people told him that 'twas no more than happen'd to them when they were so high above the plain; it being a common observation among them. And I was the more inclin'd both to make inquiry about these matters; and to believe what he said, because what he related of their being cover'd with snow, and of an odd Temperature of Air, I had learn'd before from a Travailer of another Nation than this Person, and a stranger to him.

The same Churchman, being ask'd by me, Whether he had not in some part of *Europe* made the like observation (of the difficulty of breathing) told me, that he had done it upon the Top of a Mountain in the Country of *Senenes* in or near the Province of *Languedoc*; which may serve to confirm what I am about to relate from the mouth of a Learned Travailer, that was upon the Top of one of the *Pyreneans* that is not very remote from the Mountains we speak of.

This Gentleman, who was a person Curious and Intelligent, being Brother in Law to one of the chief Lords of those parts, was by him invited, about the beginning of *September* to visit a neighbouring Mountain, that is at least one of the highest of the *Pyreneans*, which is commonly called *Pic de Midi*, upon whose Top, (where a Tent was spread for them) they stay'd many hours. His Answers to the other Questions I asked him, are elsewhere related: All that concerns this place being that

I find this set down among my Adversaria ; viz. [I also inquired of him whether they found the Air at the Top as fit for Respiration as Common Air, which he told me they did not, but were fain to breath shorter, and oftner than usuall ; and because I suspected , that might come from their motion, I ask'd, whether they observ'd it to cease, when they came down to the Bottom of the Hill, which he told me they plainly did, besides that they stay'd many hours at the Top, too long to continue out of Breath.

But that I may not here conceal any thing, that may conduce to the Discovery of the truth in the matter under consideration, I shall here add, that I did sometimes think it worth further Inquiry, Whether the Sickness, if not also the Difficulty of breathing, that some have been obnoxious to in the uppermost parts of *Pariacacha*, and perhaps some other high Mountains, may not be imputed not so precisely to the Thinness and Rarity of the Air in places so remote from the lowermost part of the Atmosphere, as to exclude certain steams of a peculiar nature, which in some places the Air may be imbued with ? In favour of which suspicion I remember, that inquiring once of an intelligent man , who had liv'd several years in the Island of *Teneriffe*, Whether he had been at the Top of the *Pic* of that name, and what he had there taken notice of about the Air ? He answer'd me, That he had attempted to go up to the Top of the Mountain, but that, though some of the Company were able to do so, he and some others, before they had reached near so high, grew so sick upon the operation they felt of the sharp Air, and Sulphureous exhalations which infected it, that they were fain to stay behind their Companions, he having already found this effect of those piercing steams upon his face (which when he made me this relation, was of a fair complexion) that the skin began to be of a pale-yellow, and even his hair to be discolour'd.

The XII. Title.

Of the Observations produced in an Animal in Changes as to Rarity and Density made in the self-same Air.

In the Experiments hitherto recited, the Animals that were recover'd from a gasping Condition, have been so, by letting in fresh Air upon them, and not the same that had been withdrawn from them. Wherefore I thought it very requisite to try, Whether the same portion of Air, without being renew'd, would, by being expanded much beyond its usual degree, and reduc'd to it, serve to bring an Animal to Deaths door, and revive him again ; since by the success of such a Tryal, it would notably appear, that the bare change of the consistence of the Air, as to Rarity and Density, may suffice to produce the abovementioned Effects.

But to devise a way to put this Experiment in practice appear'd no easy matter ; since it required a Receiver that should be transparent, & be capable of changing its bulk without suffering any Air to get in or out.

To surmount these difficulties, the first thing I thought on was, to take a fine limber and clear Bladder of a sheep or hogg, made more transparent by being anointed with Oyl, which was done on the outside, that the smell of it might less offend the Animal to be included. Then we clipp'd off as much of the Bladder at the neck, as was judg'd absolutely necessary to make an Orifice capable of letting in a *Mouse*; that sort of Animals being, by reason of their smallness, the fittest of those furnish'd with Lungs and hot blood, we could procure. And whereas it seem'd very difficult, when the neck of the bladder was cut off, to make up so large an orifice without wrinkles, at which the rarified Air may escape; to obviate this inconvenience, we provided a round stick somewhat less than the Orifice; that, the wood being laid over with a close and yielding cement, (for, pitch or the like common stuff will not alwaies serve the turn) we might be able to tye the bladder fast and close enough upon the thus fitted stopple.

And now to reduce these things to practice, and by their help make our designed Experiment, we included a *Mouse* into a Receiver made according to this way, leaving in the bladder as much Air, as we thought might suffice him for as long a time as the Experiment was to last. Then putting this limber or extensible Receiver, if I may so call it, into an ordinary one of Glass, and placing this Engine near a window, that we may see through both of them; the Air was by degrees pumped out of the external Receiver, (as for distinction sake I shall call it,) and thereupon the Air included in the bladder did proportionably expand it self and so distend the external Receiver, till being arrived at a degree of Rarefaction, which rendred it unfit for the included *Mouses* Respiration, I perceived, though with some difficulty, in this Animal the signs of his being in great danger of sudden death. Whereupon the outward Air, being hastily let into the external Receiver, compressed the swelled bladder to its former dimensions, and thereby the included Air to its former density, by which means the fainting *Mouse* was quickly revived. Having given him some convenient time of respite, the Experiment was reiterated with the like success, and we doubted not but the third tryal, we made, would have ended as the two former did; but that, whilst we were considering of the sickness of the *Mouse*, which, by reason of some opacity that could scarce be avoided in the wrinkled bladder, was not, as to its degree, so easily taken notice of, it grew irrecoverable by the subsequent condensation of the Air.

N.B. The Confirmation of this by further Experiments will properly fall under another Title.

The XIII. Title.

Of an unsuccessful Attempt to prevent the necessity of Respiration by the Production or growth of Animals in our Vacuum.

Having had frequent occasions to observe, how quickly those Animals, whose Blood is actually warm, did expire in our *Vacuum*; and that even those Animals with Lungs, whose Blood was actually cold, were

not able to live any considerable time there ; I thought it very well worth while, and yet extremely difficult, to try, whether there might not be some wayes yet unpractised, either to make such Animals as Nature endows with Lungs, live without Respiration, or at least to bring such *Insects*, and other Animals, as can already live without Air, to move also without it in our *Vacuum*.

Therefore considering with my self what happens to *Infants* and other young Animals in the Womb, and even after they come from thence, if they continue to be wrapt up in the *Secundines* ; though as soon as they are brought into the free Air they may be presently killed by being kept from breathing : Considering also, what I elsewhere relate of the slow Expiration of a very young *Kitling* in our *Vacuum* ; together with the long want of Respiration, which Custome enables some *Divers* to endure : Considering these things, I say ; though I know, that somewhat may be objected to shew, that these Instances are not altogether full to my purpose ; yet they, among other things, invited me to think, that the least unlikely projects, that occur'd to my barren Invention, would be these that follow.

First, I thought fit to try, Whether the *Seeds* of respiring Animals might be either hatched or otherwise brought to produce Young ones in our *Vacuum*. For, if that could be compassed, I should obtain my end.

Next, in case of my failing in the former attempt, and that, which is to be after a few Lines proposed, I thought fit to try, whether at least I could not bring the *Eggs* of *Insects* to hatch or be animated ; or *Aurelias* (as they call them) that were already alive, turn according to the course of Nature, into *Winged Insects*, as Flies or Butter-fishes : (of which tryals and those of the former sort, the account properly belongs to another place, where I relate the success of these and other attempts to produce Plants and Animals in our *Vacuum*.)

But *thirdly*, Considering that Nature has so ordered it, that *Froggs*, though when they are grown big enough to deserve that name, they be amphibious Animals endowed with Lungs ; yet before they attain to that pitch, they live wholly in the water like Fishes, I thought it the most expeditious and least improbable attempt we could make, to try, Whether or no this Animal, being as a Fish brought to live either in our *Vacuum*, or at least in highly rarified Air, would not continue to do so, after its Lungs should be perfectly formed. Wherefore though I foresaw and foretold the difficulty, that would be met with in the prosecution of this Experiment, namely that the Aereal Bubbles, that would be disclosed in such soft Bodies upon the withdrawing of the pressure of the ambient, would so violate the slight Texture of those tender Animals, as to hinder them from living long or moving freely ; yet I thought it very fit to attempt the Tryal, whereof I find this account among my Adversaria.

Experiment 1.

We took a good Company of *Tadpoles*, and put them with a convenient quantity of water into a Portable Receiver of a round figure, and observed, that at the first exuction of the Air they did rise to the top of the water, though most of them subsided again, till the next exuction raised them. They seemed by their active and wrigling motion to be very discomposed. The Receiver being exhausted, they continued restless moving all of them in the top of the water, and though some of them seemed to endeavour to go to the bottom, and dived some part of the way, especially with their heads, yet they were immediately boyed up again. Within an hour or a little more they were all moveless, and lay floating on the water; wherefore I opened the Receiver, upon which the Air rushed in, and almost all of them (which were many) presently sunk to the bottom, but none of them recovered to life.

Experiment 2.

A little after these, we included a lesser number of *Tadpoles* in a smaller Glass, which was also exhausted with the like circumstances with the former. And when I found the other *Tadpoles* to be dead, I hasted to these, which did not, except perhaps one, give any sign of Life, but upon letting in the Air, these having not been long kept from it, some few of them did recover, and swam up and down lively enough for some time, though after a while they also dyed.

Experiment 3.

Some years after I repeated the same Experiment in a portable Receiver of a convenient kind, and though, after the Exhaustion was perfected, the *Tadpoles* did for a while move briskly enough on the top of the water (none of them appearing able to dive or swim under water) yet coming to look on them at the end of an hour, they seemed to be all of them quite dead, yet continued floating. And though within half an hour after that, I let in the Air upon them, yet all the effect of it was, that the most of them immediately sunk to the bottom, as the rest of them did a very little while after; none of them, that I could observe, recovering any vital motion.

Experiment 4.

There remains an Experiment, which I often judged as well more hopeful as more noble, if I could procure an opportunity to bring my design to a tryal, which I have found it very difficult to do; nevertheless I was able to do it once, though not fully as I desired, yet not altogether without success.

We procured then, and with much ado, some of those odd Insects, which I elsewhere describe, whereof *Gnats* have by some ingenious men been observed to be generated about the end of *August*, or beginning of *September*. These for some weeks live all together in the water (as *Tadpoles* do) swimming up and down therein, till they are ripe for a transmigration into flies: which it self is so great a Rarity in Nature, as makes these little Creatures recompence to our Curiosity the trouble, they of-

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ten give our faces and hands. Supposing then that, if I could get some of these, and include them, being of those Insects they call *Aquatilia*, and so minute as they are, they may live a great while in the Receiver without Air, and in the mean while attain the Period, which, according to nature's course, is wont to turn them into flies, which might come forth winged Creatures into a Medium not furnished with common Air, as others of their kind enjoy ; supposing, I say, that these Insects would afford me some information about these particulars, having upon much watching met with four or five of them after a shower of Rain, that dropt from a house into a vessel laid on purpose for it, we included them with some of their water into a small Glass Receiver, which being very exactly closed, we kept in a South-window, where these little Creatures continued to swim up and down for some few dayes without seeming to be much incommodated by so unusual an habitation ; and at the end of that time, and much about the same day, they devested the habit they had, whilest they lived as fishes, and appeared with their *Exuvia* or cast coats under their feet, shewing themselves to be perfect *Gnats*, that stood without sinking upon the surface of the water, and discovered themselves to be alive by their motion, when they were excited to it : but I could not perceive them to fly in that thin medium ; to which inability, whether the viscosity of the water might contribute, I know not, though they lived a pretty while, till hunger or cold destroyed them. Some thing in this Experiment may deserve serious Reflections ; which I cannot spare time to offer at.

A Digressive Experiment concerning the Expansion of Blood and other Animal Juices.

For some purposes, relating partly to Respiration, and partly to other Enquiries, I thought fit to endeavour to obtain what information could be procured, of the Consistence and Disposition to expand it self of *Blood* and other Animal Liquors ; In pursuance of which the ensuing tryals, among others, were undertaken.

The warm Blood of a Lamb or a Sheep being taken as it was hastily brought from the Butchers, where the Fibers had been broken to hinder the coagulation, was in a wide mouth'd Glass put into a Receiver, made ready for it ; and the pump being early set on work, the Air was diligently drawn out : but the Operation was not alwaies, especially at first, so early manifest, as the Spirituousness of the Liquor made some expect ; yet this hindered not but after a long expectation, the more subtle parts of the Blood would begin to force their way through the more clammy ones, and seem to boyle in large clusters, some as big as great Beans or Nutmeggs ; and sometimes, to the wonder of the by-standing Physicians, the Blood was so Volatile, and the expansion so vehement, that it boyled over the containing Glass ; of which, when it was put in, it did not, by our estimate, fill above a quarter. Having also included some *Milk* warm from the Cow in a Cylindrical vessel of about four or five inches high, though the Operator were induced to pump a great while

before any intumescence appeared in the milk, yet afterwards, when the external Air was fully withdrawn, the white liquor began to boyle in a way that was not so easy to describe, as pleasant to behold : And this it did for a pretty while with so much impetuosity, that it threw up several parts of it self out of the wide mouthed Glafs that contained it (and could have contained as much more) though there were not above two or three ounces of the liquor.

A yet greater disposition to intumescence we thought we observed in the *Gall*, which was but suitable to the viscosity of the Texture.

Note, that the two foregoing Experiments were made with an Eye cast upon the inquiry, that I thought might be made ; Whether, and how far the destructive operation of our Engin upon the included Animal, might be imputed to this, that upon the withdrawing of the Air, besides the removal of what the Airs presence contributes to life, the little Bubbles generated upon the absence of the Air in the Bloud, juyces, and soft parts of the Body, may by their Vast number, and their conspiring distension, variously streighten in some places, and stretch in others, the Vessels, especially the smaller ones, that convey the Bloud and Nourishment ; and so by choaking up some passages, and vitiating the figure of others, disturb or hinder the due circulation of the Bloud ? Not to mention the pains that such distensions may cause in some Nerves, and membranous parts, which by irritating some of them into Convulsions may hasten the death of Animals, and destroy them sooner by occasion of that irritation, than they would be destroyed by the bare absence or loss of what the Air is necessary to supply them with. And to shew, how this production of Bubbles reaches even to very minute parts of the Body, I shall add on this occasion (hoping that I have not prevented my self on any other,) what may seem somewhat strange, what I once observed in a *Viper*, furiously tortured in our Exhausted Receiver, namely that it had manifestly a conspicuous Bubble moving to and fro in the waterish humour of one of its Eyes.

Another Digressive Experiment belonging to the same Title.

To shew, that not only the Bloud and Liquors, but also the other Soft parts, even in cold Animals, have Aerial particles latitant in them ; we took the *Livers* and *Heart* of an *Eele*, as also the Head and Body of another fish of the same kind, cut a sunder crosse wayes somewhat beneath the heart, and putting them into a Receiver, upon the withdrawing of the Air we perceived, that the Liver did manifestly swell every way, and that both the upper and lower parts did so likewise ; and at the place where the division had been made, there came out in each portion of the fish divers Bubbles, several of which seemed to come from the *Medulla Spinalis*, or the cavity of the Back-bone, or the adjoining parts ; and the External Air being let in, both the portions of the *Eele* presently shrunk, some of the skin seeming to be grown empty or flaccid in each of them.

The XIV. Title.

Of the power of Affuefaction to enable Animals to hold out in Air, by Rarefaction made unfit for Respiration.

The power of Affuefaction in other Cases, made me think it very well worth trying what it would do in Respiration: And the rather, because I presumed, it might prove an Experiment of good use, if we should discover, that by a gradual accustomedness an Animal may be brought to live, either in a much thinner Air, or much longer in the same Air, than at first he could. But in regard that to make such a Tryal perspicuously enough, the opacity of the Bladder made use of in the former Title, was like to be an Impediment, I devised another way to obviate that Inconvenience, which may, I hope, be competently understood, by the heedfull perusal of the following Tryals.

Experiment 1.

We included in a round Violl with a wide neck, (the whole Glas being capable of containing about 8 ounces of Water) a young and small Mouse, and then tyed strongly upon the upper part of the Glasses neck a fine thin bladder, out of which the Air had been carefully expressed, and then conveyed this Phantastical Vessel into a middle sized Receiver, in which we also placed a Mercurial Gage (adjusted by our elsewhere mentioned standard;) This done, the Air was by degrees pumped out, till it appeared by the Gage, that there remained but a fourth part in the external Receiver (as for distinctions sake I call it;) whereupon the Air in the internal Receiver expanding it self, appeared to have blown the Bladder almost half full, and the Mouse seeming very ill at ease by his Leaping, and otherwise endeavouring to pass out at the neck of his uneasy Prison; we did, for fear the over thin Air would dispatch him, let the Air flow into the external Receiver, whereby the bladder being compressed, and the Air in the Violl reduced to its former density, the little Animal quickly recovered.

Experiment 2.

A while after, without removing the bladder, the Experiment was repeated, and the Air by the help of the Gage was reduced to its former degree of Rarefaction, and the Mouse, after some fruitless endeavours to get out of the Glas, was kept in that thin Air for full 4 minutes; at the end of which he appeared so sick, that, to prevent his dying immediately, we removed the External, and took out the Internal Receiver. Whereupon, though he recovered, yet 'twas not without much difficulty, being unable to stand any longer upon his feet, and for a great while after continued manifestly trembling.

Experiment 3.

But having suffered him to rest a reasonable space of time, presuming that affuefaction had accustomed him to greater hardships, we conveyed him again into the external Receiver, and having brought the Air to the former degree of Expansion, we were able to keep him there for a full quarter of an hour; though the external Receiver did not at all considerably leak; as appeared both by the Mercurial Gage, and by the continuing distension of the Bladder. And 'tis worth noting, That, till near the latter end of the quarter of an hour, not only the Animal did scarce at all appear distressed, remaining still very

quiet : but, which is more, whereas, when he was put in, the tremblings formerly mentioned were yet upon him and continued so for some time ; yet afterwards, in spite of the Expansion of the Air he was then in, they left him early enough And when the Internal Receiver was taken out, he did not only recover from his fainting fit sooner then before ; but escaped those sublequent tremblings, we have mentioned.

Experiment 4.

Incouraged by this success, after we allowed him some time to recollect his strength, we reconveyed him and the odd vessel, wherein he was included, into the former Receiver, and pumped out the Air, till the Mercury in the Gage was not only drawn down as low as formerly ; but near half an Inch lower, that there the Air might be yet further expanded, than hitherto it had been. And though this did at first seem to discompose our little Beast ; yet after a while he grew very quiet, and continued so for a full quarter of an hour : when, being desirous to try what operation a further Rarefaction of the Air would have upon him, we caused three Exsuctions more to be made by the Pump, before we discovered him to be in manifest danger, (at which time the Bladder appeared much fuller then before ;) but then we were obliged to let the Air into the outward Receiver ; whereupon the Mouse was more speedily revived then one would have suspected.

And these tryals of the Power of Assuefaction seemed the more considerable, because the Air, in which the Mouse had all this while lived, had been clogged and infected with the excrementitious Effluviams of his Body ; for 'twas the same all along, we having purposely forbore to take off the Bladder, whose regular Intumescencies and shrinkings sufficiently manifested, that the vessel, whereof 'twas a part, did not leak.

P O S T S C R I P T.

Though the success of the recited Experiments is very promising ; yet a subsequent Tryal or two, whose particularities are slippt out of my memory, oblige me, in point of Candour, to declare, that, for further satisfaction, the Tryals of the power of Accustomance in reference to Air unfit for Respiration, ought to be both reiterated, and to be made in differing sorts of Animals.

The XV. Title.

Some Experiments shewing, that Air, become unfit for Respiration, may retain its wonted pressure.

Experiment 1.

We took a Mouse of an ordinary size, and having (not without some difficulty) conveyed him into an Oval Glass, fitted with a somewhat long and considerably broad neck, which we had provided, that it might be wide enough to admit a Mouse in spite of his struggling. We conveyed in after him a Mercurial Gage, in which we had diligently observed and marked the Station of the Mercury, and which was so fastned to a Wire reaching to the bottom of the Oval Glass, that the Gage, remaining in the neck, was not in danger to be broken by the motions of the Mouse in the Oval part : The upper part of the long neck of the Glass was, notwithstanding the wideness of it, hermetically sealed by the help of a Lamp and a pair of Bellows, that we might be sure, that the

the imprisoned Animal, should breath no other Air, then that which filled the Receiver at the time when it was nipped up. This done, the Mouse was watched from time to time, and though by reason of the largeness of the Vessel in comparison of so small an Animal, he seemed to me rather drooping then very near death at the end of the second hour ; yet coming to look upon him about half an hour after, he was judged by the Spectators quite dead, notwithstanding our shaking of the Vessel to rouse him up. This made me cast my eyes upon the Gage, wherein I could not perceive any sensible change of the Mercuries Station. But being unwilling to give over the Mouse without trying what fresh Air would do to recover him, I caused the sealed part of the Glass to be broken off, and, notwithstanding that his continuing to appear dead increased the confidence of those that thought him so, I obtained after a while some faint tokens of life ; though I am not sure, that they would have continued in a Vessel where the Air was so clogged and infected, if it had not been that fresh Air was frequently blown in by a pair of Bellows, whose nose was inserted into the neck of the Glass. This fresh Air seemed evidently, though but slowly, to revive the gasping Animal, whom I would not, nor could not conveniently take out of the Glass, till he had gained strength enough to make use of his Leggs ; after which without breaking of the Glass (which I was loath to loose, having then no other of the kind) we took him out and found him quickly able to go up and down. After which service, and another Tryal we had made with him, which belongs not to this place, we set him at liberty to shift for himself.

Experiment 2.

Such an Experiment as the former we made with like success upon a small Bird included *with a Gage* in a Receiver holding about a quart of Water. The Bird in about half an hour appeared to be sick and drooping, and the faintness and difficulty of breathing increased for about two hours and an half after that, at which time the Animal dyed, the Gage being not sensibly altered unless perhaps the Mercury appeared to be impelled up a little thought higher than it was when put in ; which yet might well enough proceed from some accidental cause.

Experiment 3.

To satisfy some curious persons ; that it is not want of *Coldness*, but something else in the included Air, that makes it destroy the Birds that are pent up in it, and by the hot Exhalations that steam from their bodies may be supposed to overwarm it, we made the following Experiment.

In a Glass-Viol, capacious enough to hold about 3 quarts of water, we not only included, but for greater accuracy hermetically sealed up a small Bird, and found, that in a few minutes he began to be sick and pant ; which Symptoms I suffered to continue and encrease against the mind of a learned bystander, (who thought the Animal would not hold out so long.) till they had lasted just half an hour : at which time having provided a vessel of water with *Sal. Armoniack*, newly put into it, to refrigerate it, (according to the way I elsewhere published ;) and the liquor thus made exceeding cold, somewhat to the wonder of those that felt it ; the Viol with the sick Bird was immersed in it, and kept there in that condition for 6 minutes ; and yet it did not appear, in the judgment of the

the By-standers, that the great refrigeration; that must be this way procured to the imprisoned Air, did sensibly revive or refresh the drooping Animal, who manifestly continued to pant exceedingly as before, and, as some affirmed, more; So that this remedy proving ineffectual, the viol was removed out of the water, and the Bird sometime after did, as I foretold, make many strains to Vomit (though she brought up little) followed by Evacuations downward, before she quite expired, which she did within a minute or two of a just hour, after the beginning of her imprisonment.

If I had been able (which I was not) to procure more Birds, I would willingly have prosecuted this Experiment by several other not unhopefull tryals; which for want of Subjects I was fain to leave only designed.

The XVI. Title.

Of the use of the Air to elevate the Steams of Bodies.

In the Digression about Respiration annex to the 41th of our *Physico-mechanical* Experiments formerly published, it is proposed as one of the considerable uses of the Air in Respiration, that, being drawn into the Lungs, it serves to carry off with it, when 'tis breathed out again, the Recrementitious steams that are separated from the mass of Blood in its Passage through the Lungs: from which fuliginous Excrements if the Blood were not continually freed by the help of the Air, after nature had been accustomed to that way of discharging them, their stay in the body might have very great and destructive Operations on it.

For the Illustration of this use of the Air, I shall now subjoyne the following Experiment.

We made by distillation a blood-red Liquor, which chiefly consisted of such Saline and Spirituous particles, as may be obtained from the Mass of Blood in humane Bodies; This Liquor is of such a Nature, that if a Glass Viol, about half filled with it, be kept well stop'd, the red liquor will rest as quietly as any ordinary one, without sending up any smoak or visible Exhalation; But if the Viol be unstopped so, that the external Air be permitted to come in, and touch the Surface of the Liquor, within a quarter of a minute or less, there will, upon this Contact, be elevated a copious white smoak, which will not only fill the upper part of the Glass, but plentifully pass out into the open Air, till the Viol be again stopped.

My purpose in this Tract to forbear sidings in Controversies, keeps me from taking notice of the speculations suggested by some of the Phenomena of this Liquor; which yet I thought I might lawfully mention, as far as I have done it, because it but adventures upon giving one of the Uses rather of the Air, then immediately of Respiration it self; and is brought but to illustrate what I have not found denied by any, though considered by very few; namely, the office of the Air to carry off in Expiration the fuliginous steams of the Lungs. For, in our Experiment we manifestly see, that the very Contact of the Air may give the Cor-

pufcles

pufcles of moist bodies a peculiar Volatility, or facility to emerge in the form of Steams. I know, there are some Corrofive Spirits, as in Nitre and Salt, fimple, or compounded of them, that, when they are very ftrong, emit for a while manifelt fumes ; but the difference of thofe Liquors, and their inferiority to our red Spirit, in the Capacity of fmoaking Liquors, might eafily enough be manifelted, if it were judged proper in this place, where it may fuffice, to take notice of thefe two things : The *one* is, That when the Viol has lain ftopped and quiet a competent time, the upper half of it will appear deftitute of fumes, of which the Air, it feems, will imbibe, and constantly retain *but* a certain moderate quantity, which may give fome light towards the Reafon, Why the fame Air, which will be quite clogged with fteams, will not long ferve for Refpiration, which requires frequent fupplies of frelh Air : The *other* is, That if the unflop'd Viol were placed in our *Vacuum*, it would not emit any vifible fteams at all, nor fo much as to appear in the upper part of the Glafs it felf that held the Liquor ; whereas, when the Air was by degrees reftored at the ftopcock, without moving the Receiver it felf, to avoid injuring its clofenefs, the returning Air would prefently raife the fumes, firft into the vacant part of the Viol, whence they would afcend into the Capacity of the Receiver ; and likewise, when the Air, that was requifite to fupport them, was pumped out, they alfo accompanied it, as their unpleafant fmell evinced, and the red Spirit, though it remained unftopped, emitted no more fumes till the new Air was let in.

One may compare with this Liquor another Smoaking one, mentioned in the 20th of the firft publifhed Pneumatical Experiments, where an Experiment is related of it, that has fomething in common with *this*, and may fo far ferve to confirm what is now delivered, as this alfo has fome things additional to *that*: Befides that that Liquor being made with Ingredients Corrofive, and of a bad name among Chymiffts themfelves, the fumes, that proceed from it, may fright many from daring to meddle with it : whereas this our red Spirit has been found potently medicinal for fome diftempers of the Lungs by a Dr. of *Physick*, whom I defired to try it. The other Phænomena of this Liquor I fhall not ftay to defcribe as not belonging to this place, and the Liquor it felf with very little variation I have in the *History of Colours* communicated.

The XVII. Title.

Of the long continuance of a Slow-worm and a Leech alive in the Vacuum made by our Engin.

In the often cited digreffion about Refpiration, there is mention made of the great Vivacioufnefs of *House-Snails* as they call them, and how little operation the withdrawing of the Air had upon them in comparifon of what 'tis wont to have on other Animals. I fhall now add by way of Confirmation, that I made Tryal upon ordinary *white Snails without fhells*, whereof two of differing fizes (the biggeft about an Inch and a half, and the other about an Inch in length) were included in a fmall portable Receiver, which being carefully exhaufted, and fecured againft the

the Return of the Air, was attentively considered by me, presently after 'twas removed from the Engine; whereby it was easy to discern, that both the Snails thrust out and retracted their Horns (as they are commonly called) at pleasure, though their Bodies had in the softer places pretty store of newly generated Bubbles sticking to them: but though they did not loose their motion near so soon, as other Animals were in our *Vacuum* wont to do; yet coming to look on them after some hours, they appeared moveless and very tumid, and at the end of 12 hours the inward parts of their bodies seemed to be almost vanished, and they seemed to be but a couple of small full-blown Bladders; and on the letting in of the Air they immediately so shrunk, as if the Bladders having been prick'd, the receding Air had left behind it nothing but skins; nor did either of the Snails afterwards, though kept many hours, give any signs of life.

Upon a supposition that the cold, and clammy Constitution of Snails might be a main cause of their being able to endure the absence of the Air so well, I thought it worth Tryal, whether *Efts* and *Leeches* might not yet be more able to continue in our Vacuum than a Snail; and accordingly some Experiments were made pursuant to that Curiosity; the most fully registred whereof are these that follow.

Experiment 1.

We included in a Receiver, whose Globular part was about the bigness of a large Orange, one of that sort of Animals, that they vulgarly call *Efts*: having withdrawn, but not sollicitously, the Air, and secured the vessel against the unpermitted return of it, we kept him there about 48 hours, during all which time he continued alive, but appeared somewhat swelled in his belly; his under-chap moving the very first night, but not the day and night following. By opening the Receiver at length under water, we perceived, that about half the Air had been drawn out. As soon as the water was impelled into the Glass, the Animal, that was before dull and torpid, seemed, by very nimble and extravagant motions, to be strangely revived.

Experiment 2.

We took a *Leech*, that was of a moderate bigness, or somewhat short of it, and having included it together with some water in a portable Receiver, that was guessed to be capable of holding about 10 or 12 ounces of that liquor; the Air was pumped out after the usual manner, and the Receiver being removed to a lightsome place, we observed, as we expected, that, the Leech keeping her self under water, there emerged from divers parts of her Body store of Bubbles, some of them in a dispersed way, but others in Rows or Files, if I may so speak, that seemed to come from determinate points. Though this Production of Bubbles lasted a pretty while, yet the Leech did not seem to be very much discomposed by her present condition. This done we disposed of the Receiver, which was well secured from the ingress of the outward Air, into a quiet place, where we daily visited it once at least, or oftner, as there was occasion; and

and found the Leech somewhat fastened by her tail to that part of the Glass that was under water, and sometimes wandring about that part, which was quite above water ; and still, when we endeavoured to excite her, she quickly manifested her self to be alive : And indeed (which will be thought strange) appeared so lively after the full expiration of five natural days, that expecting something might have happened to the Receiver, and thereupon resolving to try how stanch it had continued, I opened it under water, by which means the outward Air impelled in so much of that Liquor, that I was satisfied, the Receiver was immediately before as well exhausted, as others are wont to be in our Pneumatical Experiments.

The XVIII. Title.

Of what happened to some Creeping Insects in our Vacuum.

Notwithstanding the great Variety of Reptills, that Nature does almost every where produce ; yet the inconvenient time and place, wherein the following Tryals were made, supplied me with so few, that about these Animals I find among my Adversaria no more then the ensuing Notes.

Experiment 1.

We took five or six *Caterpillars* of the same sort ; but I could not tell to what ultimate Species the Writers about Insects referred them. These being put into a separable Receiver of a moderate size, had the Air drawn away from them, and carefully kept from returning. But notwithstanding this deprivation of Air, I found them, about an hour after, moving to and fro in the Receiver ; and even above two hours after that, I could by shaking the Vessel, excite in them some motions, that I did not suspect to be Convulsive. But looking upon them again some time before I was to go to bed (which may be was about 10 hours after they were first included) they seemed to be quite dead, and, though the Air were forthwith restored to them, they continued to appear so, till I went to Bed ; yet, for Reasons elsewhere expressed, I thought fit to try, whether time might not at length recover them, and leaving them all night in the Receiver, I found the next day, that 3, if not 4 of them, were perfectly alive.

Experiment 2.

We took from an hedge a branch, that had a large Cobwebb of *Caterpillars* in it, and having divided it into two parts, we put them into like Receivers, and in one of them shut up the *Caterpillars* together with the Air, which from the other was exhausted. The event was, That in that which had the Air, the little and difficultly visible Insects, after a small time, appeared to move up and down as before, and so continued to do for a day or two ; after which, other occasions made the Experiment to be neglected : whereas that Glass whence the Air had been drawn out, and continued kept out, shewed after a very little while no motion that we could perceive. But to try, Whether *Caterpillars* may continue so far alive in our *Vacuum* all the winter, as the next Spring

or Summer to proceed in the transmigration to a Butterfly, is a Tryal that we have but begun, and therefore must not pretend to say any thing about its Event.

The XIX. Title.

Of the Phenomena suggested by Winged Insects in our Vacuum.

When our Physico-mechanical Experiments were dispatched to the Press, the inconvenient season of the year, and the difficulty of making the Receivers, I then employed, to keep out the Air for any long time, hindered me from then publishing above a Tryal or two of what would happen to *Winged Insects* in our Vacuum. But afterwards being provided of more commodious Vessels, I thought fit at several times to repair that Omission by various Attempts, where of the chief ensue.

Experiment 1.

There were taken 4 middle-siz'd *Flesh-flies*, which having their heads cut off were inclosed in a Portable Receiver, furnished with a pretty large Pipe and a Bubble at the end. As soon as the Receiver was Exhausted, those Flies lost their motion (which was not brisk before;) an hour or two after, I approached them to the fire, which restored not their Motion to them (but as to one of them I suspected it had a Languid motion for a while;) wherefore I let in the Air upon them, after which in a very short time (though not immediately) they began one after another to move their Leggs, and one or two of them to walk; and having kept them all night in a warm place, when I sent one the next morning to try, if they would manifest any motion, he told me, that for a while they did, though, when I afterward rise myself, I could not perceive any motion in them.

Novemb. 12th
about 8 a Clock
at night.

Experiment 2.

About Noon we closed up divers *ordinary Flies*, and a *Bee* or *Wasp*; all which, when the Air was fully withdrawn, lay as dead, save that for a very few minutes some of them had Convulsive motions in their Leggs. They continued in this state 48 hours, after which the Air was let in upon them, and that not producing any signs of Life in them, they were laid in the Meridian Sun, but not any of them seem'd in any degree to recover.

Sept. 11th.

Experiment 3.

We put a great *Flesh-fly* into a very small portable Receiver, where at first it appeared to be very brisk and lively, but as soon as the Air was drawn out, fell on his back and seemed to have Convulsive motions in her feet and *Proboscis*; from whence she presently recover'd upon the letting in of the Air; which being drawn out again, she lay as dead: but a while after, (within a quarter, or half an hour) I perceived, that upon shaking the Receiver, she stirred up and down, (but faintly.) This was done pretty late yesterday, since whence I had not occasion to look on the Glass, till this night after Supper, when I found the fly not (whilest I stayed to endea-

Dec. the 11th.

your it) to be recovered either by warmth, or letting in the Air. A while after this Note was written, this Fly recovered; and being next morning sealed up again in that Glass, and kept 48 hours, though over the Chimney, died for good and all,

Experiment 4.

We took a large *Grass-hopper*, whose Body, besides the Horns and Limbs, was about an Inch in length, and of a great thickness in Proportion to that length: this we conveyed into a Portable Receiver of an Oval form, and capable of holding (by our guess) about a Pint of water and more, and having afterwards pumped out the Air, till by the Gage it appeared to have been pretty well drawn out, we took care, no Air should reenter to disturb the Experiment. The success whereof was this: First, Though, before the Exhaustion of the Air was begun, the Grasshopper was stirring, and lively, and continued so for a while after the beginning of the Operation; yet when the Air began to be considerably rarified, he appeared to be very ill at ease, and seemed to sweat out of the Abdomen many little drops of Liquor, which being united trickled down the Glass like a little stream, which made at the Bottom a small Pool of clear Liquor, amounting to near a quarter of a Spoonful, and by that time the Receiver was ready to be taken off, the Grasshopper was fallen upon his back, and lay as dead. Secondly, Though having a little after laid the Glass in a South window, on which the Sun then shone, I perceived some slow motions in the Thorax, as if he strained to fetch breath; yet I was not sure they were not Convulsive motions, and what ever they were, they lasted but a while, and then the Animal appeared to be quite dead, and to continue so for three hours from the removal of the Receiver. Thirdly, that time being expired, the Glass was opened and the Air let in upon him, notwithstanding which there appeared no sign at all of Life; but imagining there might be some time requisite to recover him out of so deep a swoon, I let the Glass rest in a convenient Posture, that the water that came from him might not endanger him, for a quarter or half an hour, and though I then perceived no signs of Life, yet being desirous to pursue the Tryal yet further, I caused him to be carried into a Sunshiny place, where the Beams of a declining Sun presently began to make him stir his Limbs, and in a short time brought him perfectly to Life again.

Experiment 5.

We took one of those *shining Beetles* they call *Rose-Flies*, and having included it in a very small round Receiver, which we exhausted, and though he that attended the Engine, affirmed, *April the 15th.* it struggled much whilest the Air was withdrawing, yet presently after, I could perceive but little motion (and part of that seemed almost Convulsive) and afterward going abroad, and not returning to look on the Glass till about 6 hours after, the Fly seemed quite dead, and discovered not any motion upon that of the Glass. And within about an hour after, though I let the Air rush in; yet no sign of Life ensued

ensued, neither immediately, nor for a pretty while after. So that suspecting the fly to be really dead, and yet not resolutely concluding it, though I would then wait no longer, yet three or four hours after (*viz.* about 10 of the Clock at night) I returned to the Receiver, and found the *Beetle* lively enough. Whereupon I caused the Glass to be again exhausted, and secured from the Ingress of the Air, during which time the Animal seemed to be much disquieted by what was done to it, but did not loose its motion before I went to bed, which was soon after.

Experiment 6.

About *Butterflies* I remember I made several Tryals, most of which chanced to be lost; but thus much I very well remember, that having observed them not only to live but to move longer then was expected, I chose to include divers of them in Receivers somewhat large, especially that I might see, whether in so thin a Medium some or other of them, by the help of their large wings, would be able to fly. But though, whilst the Air continued in the Glasses, they flew actively as well as freely up and down; and though after the Exhaustion of the Air, they continued to live and were not moveless; nay though at the bottom of the Receiver they would even move their wings and a little flutter, yet I could not perceive any of them to fly, by which I mean, perform any progressive motion supported by the Medium only. And by frequently inverting the Receiver (which I took care should be pretty long to let them fall from one extreame to the other,) they would fall like dead Animals without displaying their wings, though just as they came to touch the Bottom, some of them would sometimes seem to make some use of them, but not enough to sustain themselves, or to keep their falls from being rude enough.

The XX. Title.

Of the necessity of Air to the motion of such small Creatures, as Ants and even Mites themselves.

In the Experiments hitherto mentioned, the Animals, on which the Tryals have been made, were divers of them of a moderate Bulk; and others of them, though small, yet not of the least sizes that Nature afforded us. Wherefore I thought fit to annex the following Experiments, wherein I designed to examine, Whether even those minute sorts of Animals, whose bulk is thought the most contemptible, have not, as well as the greater, need of the Air, if not to make them live, yet at least to enable them to move.

A pretty number of *Ants* were included in a small Portable Receiver exhausted yesterday about noon: Between 6 and 7 in the Afternoon they seemed to be all quite dead, and the rather, because, though they were very lively just before they were sealed up, running briskly up and down the Bubble they were in; yet they grew almost moveless as soon as the Air was exhausted; and a little while after appeared more so: though I then suspected more then I since did, that they were much inconvenienced by some small glutinous substance that seemed to have got into the small Receiver from the Vapours of the Cement. When I looked on them

them at the the time lately mentioned, I opened the Glafs, whereupon the Air rufhed in ; but no fign of Life appeared for a great while in any of the *Ants* : but looking upon them this morning about 9 a Clock, I found many of them alive and moving to and fro.

It is faid by Naturalifts upon the Authority of Aristotle, that the Animal, the Greeks call Ζωον, is the minuteft of Living Creatures. But thofe of this fort being very hard, if at all, to be met with here, I thought fit to make fome Experiments upon the leaft of the Terreftrial Animals I could procure, and try, whether or no Mites themfelves, which are reputed but Living Points, and not to be taken notice of by the naked Eye to be living, but by motions which even an attentive one can fcarce difcover, ftand in need of the Air ; efpecially becaufe, in cafe they do, it may fuggelt to us fome odd Reflections upon the ftrange fubtlety and minutenefs of the Aerial particles, which muft be capable of flowing in and paffing out at the invifible and almoft in-imaginable fmall pores and other cavities of the parts of an Animal, whose entire body is reputed but a Phyfical point.

We conveyed then a pretty number of *Mites*, together with the mouldy Cheefe, they were bred in to nourifh them, into 3 or 4 Portable Receivers (which were all of them very fmall,) not much differing in fize. From all of thefe, fave one, we withdrew the Air ; and then, making ufe of our peculiar contrivance to hinder its return, we took them one after another from the Engin, and laid them by, for further obfervation. That one, which I took notice that we had referved, and in which, to obferve the difference, we thought fit to leave the Air, was fealed at a Lamp-furnace after the ufual manner of nipping up Glaffes there. This done, there remained nothing but to obferve the Event of our Tryals, which afforded us the enfuing *Phænomena* ;

1. Thofe *Mites*, that were inclofed in the fmall Glafs that never came near the Engin, continued alive and able to walk up and down for above a full week, after they had been put in, and poffibly would have continued much longer, if the Glafs had not been accidentally broken,

2. As foon as ever one of the Receivers was removed from the Engin, I looked with great attention upon it ; and though juft before the withdrawing of the Air the *Mites* were feen to move up and down in it ; yet within a few minutes after the Receiver was applied to the Engin, I could difcern in them no Life at all, nor was any perceived by fome younger Eyes then mine, whereunto I expofed them. Nay by the help of a double Convex-glafs (that was fo fet in a frame as to ferve me as a Microfcope on fuch occafions) I was not able to fee any of them ftir up and down. Nor was any motion taken notice of in the other fmall Receiver of like bignefs and fhape with mine, by them that had exhausted it of Air. And my occafions not permitting me to attend the obfervation any longer in the place where 'twas made, I took the Receiver, I had fo attentively confider'd my felf, along with me in the Coach, and having occafion to make fome ftay about an hour after, I looked upon it attentively again, but could not perceive any of the *Mites* to ftirr ; and the like unfulfilling obfervation I made when I had a conveniency 2
or.

or 3 hours after that. And the place I did it in being one, where I thought my self as it were at home, I first let in the Air, to try if the Mites were not quite dead, and though neither upon its rushing in, nor during my stay there, I could perceive any of them to stir; yet I left the Receiver unstoppt as it was in the Window, upon a suspition that the Air might not be able to produce its operation upon them in a short time.

3. And therefore passing by the same place about 2, or 3 days after, I called in to look upon my Receiver, and found a number of my little Animals revived, as an attentive Eye might easily perceive by the motion of certain little white specks, when it was helped to observe it by little marks, I made on the outside of the Glass (which was purposely chosen thinn and clear) near this or that Mite, with a Diamond; by the approach to, or Recess from which Marks, the progressive motion became (perhaps within a minute) plainly discoverable, especially if we used the following Expedient, (which I found the best of those I try'd,) namely, That, when the Eye perceived little white specks that looked like Mites, the Receiver should be so turned and returned, that the bellies and feet of those little Creatures were uppermost, notwithstanding which they would not easily drop down, but continue their motion; which specks being made upon the Concave surface of the thin Glass it self (to which you may approach your Eye as much as you please) are thereby rendred much more easily visible. But, this being only intimated upon the By, I proceed to take notice, that in the newly mentioned Receiver the Mites did, by stirring up and down, continue to appear alive for 2 or 3 days after, if not longer. I should not, I confess, have thought it ridiculous to suspect, that the Mites, which at first lost their motion, did at last really dye, and that those, I after saw stirring up and down, were others newly generated in the included mouldy Cheese: But I was not apt to think this suspition probable; not only because of the extream difficulty of making any living Creature to be generated in *Vacuo Boyliano*, but because it did not seem agreeable to what I elsewhere noted about the way and time of the Propagation of Mites, whose Eggs I have divers times observed with pleasure, that at a season of the year, that was not favourable (for these things happened in a cold *March*,) newly generated Mites should in 2 or 3 days grow up to their just bigness, which several of those, we observed, seemed to have attained.

4. But because it doth not by the third *Phænomenon* appear, Whether or no, in case our Mites had been kept in a moveless state for a much considerabler time then 3 or 4 hours, they would have been recoverable by the admission of the Air; I shall add, to satisfy that doubt, That one of the Portable Receivers above-mentioned, being exhausted and carefully secured from the regrefs of the Air, was kept from Munday morning to Thursday morning: after all which time, our attentive Eyes being unable to discover any signs of Life among the included Mites, the Air was let in upon them, and, after no long time, had such an operation upon them, that both I and others could plainly see them creep up and down in the Glasses again.

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